

ON "TENDON-REFLEX" AS AN AID TO DIAGNOSIS IN DISEASES OF THE SPINAL CORD.

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EPILEPTIC.

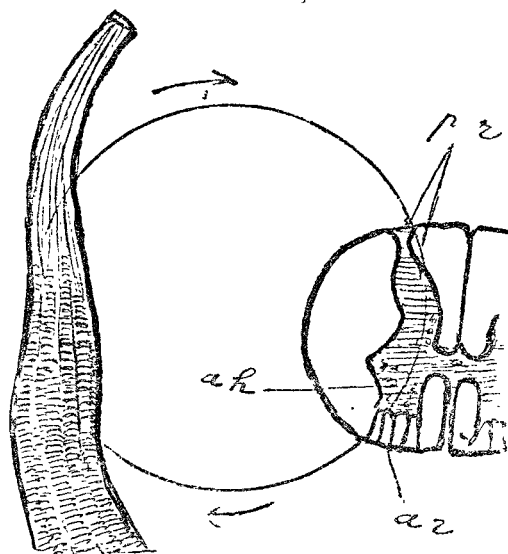
IN the following remarks it is provisionally assumed that the contraction of a muscle which follows a blow upon its tendon is of the nature of a reflex action. The adequacy of such an explanation is, I am well aware, contested by high authorities in physiology. Measurements of the interval of time which elapses between the blow and the contraction are thought to prove that this is too short for the phenomenon to be reflex. I cannot as yet consider this to be a conclusive objection, for whilst, of course, acknowledging the respect which is due to attempts at accurate measurement, experience shows that, even in very careful hands, where the question is one of delicacy—in the present instance it is a matter of a very few hundredths of a second—sources of fallacy are apt to occur in the experiments which vitiate the conclusion. It is quite conceivable, moreover, that a shorter period of time than is commonly needed for a reflex event is requisite for conduction through the centre, where, as in the present instance, the centripetal stimulation is in all probability unusually strong. Nor can we properly compare it with the time required for reflex from the skin, since the fact that in tabes, for example, whilst the skin-reflex is usually preserved intact, the tendon-reflex is always lost, shows that the conduction must, at least in some part of the course, take place through a different path.¹ At any rate, it seems advisable that at the present juncture the clinical facts should have due prominence, and there can be no question, I think, that they make out a strong *prima facie* case in favour of the reflex character of the muscular spasm. With the view of aiding in the consideration of the question from this point of view some rough diagrams are reproduced, which I have been long in the habit of using in order to illustrate the connexion between certain pathological changes in the spinal cord and the absence or exaggeration of "tendon-reflex." For the sake of simplicity, the remarks will be confined as far as possible to "patellar tendon-reflex" and "ankle clonus" as representatives of phenomena which may, as is well known, occur in connexion with other tendons and muscles.

The patellar tendon-reflex may be conveniently tested, in most instances, by the following procedure, which differs from that usually employed. The patient, who is seated, plants his foot firmly down at such a distance that the leg forms a little more than a right angle with the thigh. Whilst the observer rests the palm of his left hand upon the patient's thigh, he strikes with some implement held in the right hand several blows upon the ligamentum patellæ at about an eighth of an inch or so below the knee-cap. I nearly always employ for this purpose an ebony stethoscope, with a very heavy ear-piece and an india-rubber cord-like ring sunk into a groove on its margin. The quadriceps muscle can be felt, and (if the patient is in masculine garments) can be seen to contract more or less vigorously in response. If it be desired to ascertain the mere presence of the "reflex," this is sufficient. If, however, the response is very small, or is doubtful, or does not occur at all, the thigh as well as the leg must be bared, and other measures adopted, before a conclusion is arrived at. The leg should be crossed over the other, or the patient should be made to sit upon a table with his leg dangling. In either of these positions the presence of the "reflex" will be shown by the foot being jerked more or less vigorously forwards when the ligamentum patellæ is struck. In the case of a patient undressed, and lying on a bed, the knee should

be flexed, and the heel held firmly down upon the bedding whilst the blow is given on the ligament. The contraction of the quadriceps is readily seen.

Supposing that careful examination has established the absence of the phenomenon, we should next endeavour to ascertain at what point in the arc of nervous communication the break occurs by which the process is interrupted. Fig. 1 shows the normal condition, as it may be supposed

FIG. 1.]



to be if the phenomenon be reflex. Impulses started by a blow on the tendon after traversing the afferent nerve, enter the cord in the posterior root, *pr*, and reach the anterior horn, *ah*, where they give rise to efferent impulses, which arrive by the anterior root, *ar*, at the muscle, and cause it to contract. From the tendon to the anterior horn is the sensory portion of the arc, whilst the motor part includes the anterior horn, anterior root, and intra-muscular nerve. The integrity of this nervous arc ensures the response of the muscle (provided the muscle itself be healthy) to a blow on its tendon. So also the fact that the quadriceps is found to contract freely when the ligamentum patellæ has been struck *almost always* signifies that the nervous arc is nowhere seriously interrupted in that part of the spinal cord which gives origin to the lumbar plexus. It is not, however, quite conclusive on this score, for a reason to which I shall refer later. If, on the other hand, careful testing shows the phenomenon to be absent, it is evident that there is a "fault" (as the miners say) or break at some point or other in the continuity of the nervous arc pictured in the diagram. Should the patient who presents this abnormality be walking freely about, and able to flex and extend his knee-joint with vigour, we may at once absolve the *motor* portion of the nervous arc from the suspicion of being the seat of interruption. Were it otherwise, and the break dependent on lesion in this direction, the "fault" must necessarily be either in the muscle itself (as, for example, from pseudo-hypertrophic paralysis), in which case there would be a very evident enfeeblement, or else either from lesion of the anterior root of the spinal nerve or atrophy of the large ganglion cells in the anterior horn. In each of these latter conditions there would be not only more or less muscular powerlessness, but also a considerable amount of muscular atrophy.

But the patient may have kept his bed for some time, and is weak and more or less emaciated. In this case it becomes necessary to resort to other expedients to ascertain the integrity of the motor part of the arc. If the knee-joint be capable of voluntary flexion and extension with a fair amount of force, it will be sufficient to percuss with some instrument the vastus internus where it projects just above and to the inner side of the knee-joint. This may be done with the thin ear-piece of an ordinary stethoscope.² If this blow, in these circumstances, excites a wave of muscular contraction, it may be taken as corroborative evidence that the muscle, motor nerve, and anterior horn are free from lesion. Should, however, there be a very powerless state of the limb, the occurrence of a wave of contraction in response to a blow

¹ The literature of the subject has rapidly become extensive, and includes, besides the work of Westphal and Erb, to whom is due the credit of inauguration, most important contributions by Tschirjew, Grainger Stewart, Gowers, Hughlings Jackson, Bramwell, Charcot, Brissaud, and Berger, amongst many others. There is an able article by Dr. Augustus Waller in *Brain*, Part X., in which the physiological side of the question is discussed, and the objections to the theory of reflex action are very forcibly put.

² Hawksley has made for me an ebony stethoscope, provided with a thick and also a thin ear-piece, which are interchangeable. The former is for percussing the tendon, the latter being used for the muscular structure.

upon the muscular structure of the vastus internus will not be sufficient to prove the integrity of these structures. I have found—and I do not think this fact is generally known—that a muscle which is cut off from its trophic centre either by lesion of a spinal nerve or atrophy of ganglion cells in the anterior horn of the cord, and which fails to contract to the strongest induced currents, will yet (provided it has not lost also the power of contracting to a slowly interrupted voltaic current—the reaction of degeneration) respond by a very palpable wave of contraction to blows upon it. So that if you strike a muscle and get a wave of contraction, all that you can predicate of it is this, that the muscle will respond to *some form* of electric excitation, not that it will necessarily respond to faradaism. A muscle which thus responds to a blow may be perfectly normal, or, on the other hand, it may be far advanced towards a state of irreparable degeneration. In the latter case, however, the necessarily powerless condition of the muscle secures you from error.

It must be remembered that the patellar tendon-reflex is lost somewhat easily, and as the result of a small amount of structural change. It will often be found wanting when there is but a slight lowering of faradaic excitability. It is best therefore, whenever there is the slightest doubt about the matter, to test very carefully by induced currents the reaction of the vastus internus, the rheophore being applied to its motor point. The importance of this is evident. If by any want of care in examination we jump hastily and erroneously to the conclusion that the motor side is not in fault, we are driven to the necessity of ascribing the break to a lesion on the sensory side, and the diagnosis is consequently ruined.

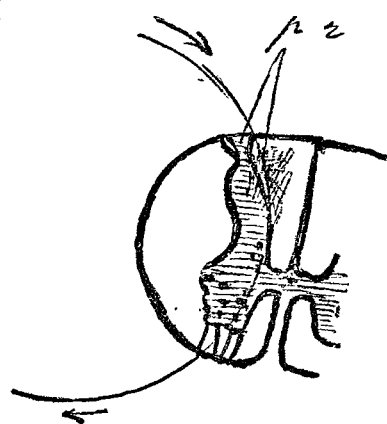
We will consider that no such want of care has been shown, and that the examination excludes the muscle, motor nerve, and anterior horn of grey matter from being the cause of the break in communication. We have now to fall back upon the grey matter of the posterior horn, the posterior root zone, and the posterior root. In one or other of these structures the "fault" must lie. Cases in which the patellar tendon-reflex is absent in health are so extremely rare that this possibility may practically be disregarded—at least until every other explanation has been exhausted.

Through the kindness of my colleague, Dr. Ramskill, I am able to refer to a case of extreme interest bearing upon this point. The patient is a young man who is affected with loss of power in the left lower extremity. There is no loss of sensibility, but very extensive wasting of the muscles of this thigh, and their excitability by induced currents is lowered. The right lower extremity is free from paralysis or wasting; he can flex and extend it vigorously, and the muscles respond normally to faradaic currents. In this limb, however, there is cutaneous anæsthesia to a very marked extent. He presents, therefore, a typical example of Brown-Séquard's hemi-paraplegia. There is a lesion, which circumstances show to be probably syphilitic, occupying more or less definitely the left half of the spinal cord in the lumbar region, and giving rise to motor paralysis on the side of the lesion, and anæsthesia on the opposite side. The occurrence of anæsthesia, in these circumstances, in the limb opposite to the lesion was explained by Dr. Brown-Séquard, it will be remembered, by the fact that the conductors of sensitive impressions from the trunk and limbs decussate in the spinal cord, so that an injury or disease of the left side of the cord—e.g., affects the conductors from the right side of the body below. Now in this patient the patellar tendon-reflex is absent in each leg, not only in the left, which is paralysed and greatly wasted, but in the right also, in which the motor power is intact, but the skin anæsthetic. To explain its absence from the left (paralysed) limb it is evident that the "fault" must be in the anterior horn of the left side of the cord, in the motor portion of the nervous arc. In the right limb, on the other hand, it is lost in connexion with lesion of some part of the sensory tract, the exact situation of which it is not easy to indicate. It seems probable that the lesion of the cord (myelitis), although greatly confined to one segment, is not absolutely so, but, whilst sparing the right anterior horn, encroaches to some extent upon the right posterior horn, and "cuts" the communication at this point. The case is a most interesting example of the fact that the absence of the knee phenomenon may be due to lesion in either the motor or the sensory portion of the nervous arc, both modes being exhibited in one individual. In certain cases—as, e.g., where there is a

diffuse myelitis involving the whole transverse section of the cord—it may be due to both.

The most common and conspicuous illustration of a flaw on the *sensory* side destroying this reflex, is in the case of *tabes dorsalis*. Fig. 2 shows the position of the essential lesion in *tabes dorsalis* in the posterior root zone in that portion—i.e., of the posterior column adjacent to the posterior horn, which is largely composed of posterior (sensory) root-fibres making their way vertically to the grey matter above. Limitation of the lesion to this spot explains at once the almost constant connexion of lost reflex with the occurrence of lightning pains, whilst the integrity of the anterior horn and anterior root explains the fact that the quadriceps muscle is neither diminished in bulk

FIG. 2.



nor materially weakened as well as that it usually responds perfectly to faradaic currents, and also to blows upon its structure. In my earliest published reference to the subject of tendon-reflex, I remarked of a case of *tabes*,³ "The muscle is here, indeed, more than normally ready to contract upon direct local stimulation." Greatly extended experience since then has shown me that a condition of exceptional irritability to percussion is very common, perhaps indeed more frequent than not in *tabes*. Erb has since recorded a similar observation.⁴ He writes, "In striking contrast to the loss of tendon-reflex, the mechanical irritability of the quadriceps is always completely preserved, and is frequently even very pronounced and lively." I showed the other day that in an elderly man with typical *tabes* in whom there remained not a vestige of tendon-reflex, the irritability to direct percussion of the vastus internus far exceeded that exhibited in the ward attendant, a young and healthy man with a powerful muscular system. An explanation of this increased excitability is probably to be found in the irritative lesion of the posterior root-fibres, the cause of the lightning pains. The symptom must be classed indeed, I submit, in the same category with the tendency to cramp in sciatica, and the clonic spasm of facial muscles which is so apt to occur in severe trigeminal neuralgia.

It occurred to me a few days ago to examine in reference to this subject a patient whose supra- and infra-orbital nerves have been recently stretched for severe neuralgia of the two upper divisions of the trigeminus on the right side. The patient was for a time greatly relieved, but is subject to occasional paroxysms of pain, and there yet remains a considerable amount of clonic spasm of the muscles on that side of the face. There is manifestly an abnormally irritable condition of the muscular tissue. This irritability, however, does not extend to the tendons. Repeated testing by percussion showed that the contraction of the zygomaticus major of the *left* (sound) side of the face, which followed a blow upon its tendinous origin on the malar bone, was distinctly much more energetic than that of the corresponding muscle on the *right* (affected) side. My reason for selecting this case for observation on this point was as follows. As is well known, the sensory supply to the face is derived from the trigeminus, the motor influence from the portio dura. Now, the sensory portion of the fifth is acknowledged to represent the posterior root of a spinal nerve. There had therefore been in the operation of stretching a kind of vivisection (with a therapeutical, not a physiological, object, as it happened) performed upon what is practically the posterior root of the nerve supply to the muscles of the face. In no other part of the body could the operation have been so limited to this effect; in the spinal nerves the posterior and anterior roots are united in one trunk. Now, supposing that nerve-fibres from the tendon take their course to the centre in the posterior root, as Tschirjew has stated, I ought to find, it appeared to me, the response to stimulation of tendinous fibres on the side on which the "posterior root" had been stretched, and therefore injured, less active than on the sound side. I had some difficulty in selecting a muscle

³ THE LANCET, July and August, 1878.

⁴ Zur Pathologie der *Tabes dorsalis*, Deutsch. Arch. f. klin. Medicin, Marz, 1879.

upon which the experiment could be tried, but the zygomaticus major showed it very well, although on a necessarily small scale. It is of no small interest to observe that although the muscular tissue was generally over-irritable (like the muscle in tabes) the response to a tap on the tendon was distinctly less vigorous than that on the sound side. I do not wish, however, to draw positive conclusions from a single observation of this kind. It would be well to repeat the examination in a patient affected with a destructive lesion of the fifth nerve, or in whom the infra-orbital trunk had been divided for neuralgia.

(To be concluded.)

ON CHIAN TURPENTINE AND ITS USE- LESSNESS IN CANCER.

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IN his second article "On Chian Turpentine and its Use in Cancer" (in THE LANCET, Oct. 2nd, 1880), Prof. Clay says, "Certain results obtained by the use of a remedy almost forgotten by the therapist, were placed by me unreservedly before the profession; and for the benefit of suffering humanity, at least, a fair trial of the remedy ought to be given before condemning it."

With what willingness a fair trial of this drug was entered upon and perseveringly continued at the Middlesex Hospital, and with what gladness we would have welcomed any measure of success with it, will be very readily believed by all who have to follow cases of cancer along their melancholy course to their tragical end. Moreover, the large opportunities for watching cancer of all kinds afforded by a hospital which, besides receiving cases of cancer for operation into its general wards, has a special cancer out-patient department open to all comers, and thirty-two specially endowed beds, which may be retained until the occupants are "*relieved by art or released by death*"; the tradition which makes it an essential part of the duty of the surgeons of the Middlesex Hospital to do whatever within them lies for the relief or cure of cancer; together with a large share in the universal desire that a remedy for this frightful, and frightfully common, malady may be discovered—naturally incline us to test the value of anything which seems to offer even the faintest ray of hope. Perhaps, however, it must be admitted that past experience of acetic acid, bromine, condurango root, and various other external and internal methods of treatment, deprives us, at the outset, of that enthusiasm of great expectations which some might possibly indulge.

But besides testing remedies, it is also expected that we duly lay before the profession the results, and it is on this account that I publish brief notes of the following cases.

In answer to those who have hitherto questioned the efficacy of Chian turpentine, Professor Clay has replied: (1) That "in a large percentage of cases factitious turpentine only has been used in the treatment of cancer"; (2) that a sufficient time has not been allowed to elapse for the full effects of the remedy to be manifested; and (3) that the full effects of the pure turpentine have not been gained in some instances because, owing to its incomplete compounding, the drug has not been entirely digested.

It is therefore necessary that I should state: (1) That the turpentine used in the following cases was at first some which was in stock at the hospital, and answered thoroughly to the descriptions of the genuine, pure turpentine. When this was exhausted, more was obtained from two well-known houses, and it was a sample of one of these which was publicly condemned by Professor Clay. Owing to the Professor's letter to the *Med. Times and Gazette* (May 15th), the use of these was at once stopped, and another supply was obtained, at his recommendation, from the Apothecaries' Company. Lastly, in the first week of October I commenced to use some placed at my disposal by a

friend, to whom it had been sent by Professor Clay. It is this which I am still using, and have been doing ever since I received it. (2) As regards the length of time the drug has been administered, I can only say that in several of the cases, if the time has been insufficient, it has not been my fault or that of the patient, but is due to the fact that the disease continued to run its course, uninterrupted by the drug, to its fatal end. (3) Both the pills and the mixture have been made in strict accordance with Professor Clay's directions by Mr. Challice, our skilful dispenser, who had, prior to Professor Clay's last article, pointed out the way to avoid any separation of the resin.¹

Professor Clay's confidence in the remedy having increased, he says he is justified "in stating that uterine cancer at least may be removed by the use of Chian turpentine;" and adds: "Most of the cases of cancer which have presented themselves are in an advanced stage of the disease, and more especially is this the case in cancers of the uterus. Yet the treatment continues to be very efficient." He has, too, put on record marvellous benefit accruing to four of his patients in such a very short time as four days, seven days, fourteen days, and sixteen days respectively, and has besides given one admirable result in a woman who, four months before, had been told at the Middlesex Hospital that "her case was incurable and that she had not long to live,"² but who after undertaking the journey to Birmingham became an in-patient at the Queen's Hospital, and was so much improved by the Chian turpentine treatment that in nine weeks she was "about to return to London with every prospect of soon being cured." None of the following cases, therefore, are open to objection on the ground that the disease was too far advanced when the turpentine was commenced; nor can the genuineness of the drug, or its method of administration, be fairly called in question.

CASE 1. Carcinoma of the fundus of the uterus; destruction of the whole organ; death.—Ann H—, aged sixty, a cook, was admitted into Stafford ward on February 2nd, 1880. Single, and had never borne a child. General health always good, except that she had suffered greatly at her menstrual periods. Catamenia commenced before the sixteenth year, had always been scanty, and ceased altogether at fifty. No cancer in family. At the beginning of January, 1879, she began to feel very weak and unequal to her work. In August, 1879, bearing-down pain in the vagina commenced; and intense hæmorrhage, which had occurred once or twice previously, now became worse. Was treated at the Great Northern Hospital in September, 1879; since then the hæmorrhage has ceased; but soon afterwards an offensive whitish discharge commenced.

On admission she was fairly nourished, but pale looking; was cheerful in manner; but said she had lost flesh, as she was at one time very stout. Suffered greatly from dragging pain in the pelvis, and had a thick, offensive leucorrhæal discharge.

Vaginal examination.—There is no ulceration of the vagina or of the os or cervix of the uterus, but an indurated and uneven enlargement of the fundus.

Sedatives, with tonic and laxative medicines, were administered during February, March, and part of April; but on April 16th Chian turpentine, in the form of mixture, was commenced, and taken three times a day without interruption until the beginning of August, when it was omitted for a time on account of sickness.—May 4th: the pain complained of is severe and frequent, more especially after sitting up. Getting out of bed is followed by hours of severe pain. Discharge considerable and blood-stained.—May 11th: The discharge is sometimes quite black; it has increased the last eight or nine days, but is not so offensive.—May 21st: Pain still continues severe; takes opium or morphia twice daily, to relieve the pain over which the Chian turpentine mixture seems to have no influence.—June 25th: Still no ulceration to be felt per vaginam, but there is considerable and painful œdema of the genitals.

During September the Chian turpentine mixture was again taken, but irregularly, and only as it could be tolerated by the stomach. On Oct. 2nd it was again commenced to be taken regularly three times a day, and was so continued

¹ Vide Pharmaceutical Journal, June 26th, 1880.

² Professor Clay having courteously informed me of the name of this patient, I asked Mr. Fardon, the resident medical officer, to refer to her case, but though he has searched both the in- and out-door cancer registers for two years, and the obstetric registers for the last year, her name does not appear. As Professor Clay adds, the statements of patients are not always to be relied upon. It seems more than probable that in this instance he was misinformed.